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A LETTER,

ADDRESSED TO

DR JOHNSON,

EDITOR OF THE LONDON MEDICO CHIRURGICAL REVIEW;

BY

DR HAMILTON,

PROFESSOR OF MEDICINE, MIDWIFERY, &C., IN THE UNIVERSITY
OF EDINBURGH.



SIR,—It was only on the 26th of October that I saw the number of your Medico Chirurgical Review for July, which contains a critique on the First Part of my Practical Observations. Had that article appeared in an anonymous publication I should certainly have paid no attention to it, but coming out under the sanction of your respectable name, I fear that certain observations, dispersed through the review, may mislead inexperienced practitioners, and impair my usefulness to the public, and to the profession, which is the great object I have had in view in the publication. I feel myself, therefore, called upon to address you on the subject.

It may be necessary to premise, that although I hold you to be legally and morally responsible for every article in your quarterly work, I do not believe that you are personally the author of the review of which I have reason to complain. I presume that, since you do not practise midwifery, you apply to some gentleman engaged in that profession for critical remarks on books in that department.

Under this impression I shall take the liberty, in the observations I have to offer, to address myself to the reviewer, but before doing so, I must explain what, in my opinion, you, as editor, ought to have required from any one whom you might have employed to review a work such as mine.

My object in the publication of that work, as explicitly stated in the advertisement, is *to record those deviations from the established modes of practice in midwifery, which the experience of nearly half a century has led me to adopt and to recommend.*

The deviations alluded to were originally occasioned by frequent disappointment in the efficacy of the established means—subsequently they were suggested by reasoning—and finally, their utility has been confirmed, not only by my own experience, but also by that of many talented individuals in different parts of the world. Accordingly, in the work reviewed in your publication, I have candidly explained this progress, shewing the inefficacy of the ordinary practice, stating the reasoning which led to the deviations I had adopted, and illustrating the utility of such deviations by an appeal to acknowledged facts, or by a detail of cases.

In reviewing a work of that description, it must be evident, that the person employed to do so, if his intentions were upright, should consider the doctrines of the author only in two points of view, viz., in reference to the utility of the old established practice, or to the reasoning on which the deviations recommended had been founded. As the facts and cases in illustration are recorded upon the solemn

testimony of the author, their accuracy cannot be questioned unless it can be proved that he is either incompetent to understand them, or that he has wilfully misrepresented them ; and I venture to believe that the reviewer dare not bring forward such an allegation.

After I shall have shewn what your reviewer has actually done, I shall leave you to judge whether he has been guided by such obvious and upright rules of conduct.

I.—In page 41, the reviewer has called in question a fact which is so well known in this city, that it must excite surprise here that any practitioner should be ignorant of it, viz., that the most certain method of distinguishing dropsy of the ovarium from ascites, is by tapping. In the former disease, the fluid drawn off is ropy and gelatinous, and after the operation, the collapsed sac can be as plainly distinguished through the parietes of the abdomen, as the collapsed uterus after child-bearing.

These facts must be familiar to every surgeon's pupil in large hospitals, and the reviewer may have an opportunity, if residing in London, of verifying them every week.

But although these facts be obvious, and, notwithstanding the scepticism of the reviewer, be incontrovertible, their practical importance can only be appreciated by those who witness the anomalous and complicated cases of disease which not unfrequently occur. For example, above fourteen years ago, an individual was subjected for several weeks to a severe course of mercury and foxglove, in the belief that she laboured under ascites. When I was called in I directed tapping, and, by the marks alluded to, I convinced her medical attendants that her disease was ovarian. She was treated accordingly, and is now alive and in good health. Will the reviewer allege, that if the nature of the case had not been ascertained, this fortunate result could have happened? Would it afford him any satisfaction to prevent this fact from being believed and appreciated by young practitioners?

II.—The next remark I have to offer relates to what is contained in your 43d page. The reviewer says, “ He (meaning Dr Hamilton) admits that in some pregnant women there may be periodical bloody discharges from the vagina ; but he qualifies this admission by stating, that there is always an irregularity in the date of the recurrence,” &c. In the 137th page of my work, there is the following strong denial of my belief in the possibility of “ *periodical* bloody discharges during pregnancy.”

“ That, in some individuals a flow of blood is directed to the uterine arteries, during the first months of pregnancy, exactly at the regular periods of menstruation, indicated by a bloody discharge per vaginam, has been often alleged, but it does not consist with the experience of the author, that there ever was, strictly speaking, such an occurrence.”

Not satisfied with having suppressed this passage, the reviewer has thought fit to substitute a different word for that which I had used, and

in making this substitution, he has selected a word which not only misrepresents my opinion, but also renders the sentence ludicrous. How can periodical bloody discharges take place, if “there be an irregularity in the date of the recurrence?” The reviewer has substituted *periodical* for *IRREGULAR*, which is my expression.

III.—The next remark of the reviewer, to which I must advert, relates to the change on the mammæ, in consequence of impregnation. The reviewer says, “Our own experience, and, we may add, that of almost all the latest and best writers on Midwifery, and on Medical Jurisprudence, do not certainly warrant, that unqualified and unequivocal reliance on the changes of the areola, as a sign of early pregnancy, which Dr H. puts in them.”

With every respect for writers on Medical Jurisprudence, I certainly cannot bow to their authority on a practical point of this nature, and so far from “all the latest writers on Midwifery” disregarding this test, one of the latest

and best writers, Dr Montgomery of Dublin, has described this change round the nipple with a degree of minuteness which could only be suggested by truth, and has expressed himself in regard to the value of this test, in as strong language as I could have done. He says, “There is, however, one of those changes which, if carefully observed, is of the utmost value as an evidence of pregnancy, which, according to our experience, can alone produce it—we allude to the altered condition of the areola.”

After noticing and quoting the authority of Roederer on this subject, he proceeds,—“The several circumstances here enumerated at least ought, in all cases, to form distinct subjects of consideration, when we propose to avail ourselves of the condition of this part as an indication of the existence or absence of pregnancy. One other we shall add as equally constant, which is a soft and moist state of the integument, which, together with its altered colour, gives us the idea of a part in which there is

going forward a greater degree of vital action than is in operation around it ; and we not unfrequently find that the little glandular follicles are bedewed with a secretion sufficient to damp and colour the woman's inner dress. We must recollect, also, that these changes do not take place immediately after conception, but occur in different persons after uncertain intervals ; we must therefore consider, in the *first* place, the period of pregnancy at which we may expect to gain any useful information from the condition of the areola."

"We cannot speak very positively as to what may be the earliest period at which this change can be observed, but we have certainly been satisfied of its existence at the end of the second month, at which period the change of colour is by no means the most distinct character to be observed, but the turgescence of the nipple, and the development of the little glandular follicles, are the objects which should principally engage our attention ;—the colour at this period being, in general, little more

than a deeper shade of rose or flesh colour, slightly tinged with a yellowish or brownish hue. During the progress of the next two months, the changes in the areola are, in general, perfected, or nearly so ; and it then presents the following character :—A circle around the nipple, whose colour varies in intensity according to the peculiar complexion of the individual, being generally much darker in persons with black hair, dark eyes and sallow skins, than in those of fair hair, light coloured eyes, and delicate complexions. The extent of this circle varies from a diameter of an inch to an inch and a half, and increases in some, as pregnancy advances, as does also the depth of colour.”

“ In the centre of this circle, the nipple is observed partaking of the altered colour of the part, and appearing turgid and prominent ; and the part of the areola more immediately around the base of the nipple has its surface rendered unequal by the prominence of the glandular follicles, which, varying in number from twelve

to twenty, project from the sixteenth to the eighth of an inch ; and lastly, the integument covering the part is observed to be softer, and more moist than that which surrounds it, and the breasts themselves are, at the same time, observed to be full and firm, at least more so than was natural to the person previously. Such we believe to be the essential character of the true areola, the result of pregnancy, and that, when found possessing these distinctive marks, it ought to be looked on as the result of that condition alone, no other cause being capable of producing it.

Dr Montgomery and I have taken our description of these phenomena from actual observation. We agree accurately in the main points. But I readily admit that his description is more minute and particular than mine. When I published my First Part, I had not seen Dr Montgomery's Observations on the Signs of Pregnancy ; but when I did see them, I wrote to the Doctor, that I should in fu-

ture add his description of the areola to my own. *

The importance of establishing the reality of this curious change on the mammæ, which Dr Montgomery has so well described, may be best understood by considering a not unfrequent case.

It is well known that practitioners are sometimes called to cases where unmarried women of good character have been obstructed for some months. No prudent person would venture to prescribe in such cases, without endeavouring to ascertain whether the young woman might not be pregnant. Suppose the symptoms to be equivocal, would the reviewer propose to apply the stethoscope to the naked belly of the woman? He may be assured that, in this part of the world at least, such a proposal would be indignantly rejected by

* *Vide Cyclopædia of Practical Medicine*, vol. iii., page 473.

every young woman of reputed respectability. But if, by obtaining permission to look at one of the mammæ, he could, by the state of the areola, determine the nature of the case, would not this be a matter of great importance?

Admitting that early pregnancy may be discovered by mediate auscultation, the knowledge of this circumstance can very seldom be applied to practical purposes; for, if an unmarried woman be conscious that she may be pregnant, she could so act with the muscles of the belly or limbs as to prevent the stethoscope indicating the condition of the uterus.

IV.—In allusion to my directions for the treatment of a particular cause of protraction of the first stage of labour, the reviewer has expressed himself in the following terms, page 47:—"The treatment which Dr H. recommends in such a case, consists in drawing blood, if the state of the patient will permit, in exhibiting an opiate enema; and after this, in introducing the finger above the stricture,

and in pressing from within outwards on the resisting band of the uterus during each successive pain."

"The last clause of the Professor's advice must be acted upon with great circumspection and gentleness. We are great enemies to all unnecessary manual interference in the management of labour, and we should be rather unwilling to carry the finger within the cervix uteri, above the seat of the stricture, for the purpose of pressing upon it from within."

"We prefer the method recommended by Dr Burns, of making the pressure merely on the anterior edge of the os uteri, as already explained in an extract given above."

Dr Burns has not alluded to that rare deviation in the phenomena of the first stage of labour, where an undeveloped band of the cervix uteri prevents the dilatation of the orifice, and therefore it is not correct to appeal to his authority. If Dr Burns were called to such a case in real practice, he would have too much good sense

to apply the pressure to the outer edge of the os uteri, for the resistance to be overcome is generally almost an inch above that part.

V.—The next subject in which I must express my strong sense of the want of candour of the reviewer, is in the following words, page 48.—“ We wish that we could with propriety say as much for the succeeding chapter on the management of the second stage. Thirty pages of print to be occupied with recommending the accoucheur to apply his naked (without a napkin) hand to the perinæum, when the head is pressing down, and to smear the parts with soft lard. In truth, this is the sum and substance of the whole chapter.”

Perhaps it would be difficult to find in the annals of medical criticism, a more uncandid and unfounded assertion than that contained in this sentence. My directions for the management of the second stage of labour are comprised in *twenty-seven lines* instead of thirty pages. But I felt it necessary, in order to

explain my own practice, to allude to that of others, and I have in consequence been obliged to quote the directions of Dr Denman, of Dr Davis, of Professor Burns, of Mons. Baudelocque, and of Mons. Velpeau, for it would have been a very unsatisfactory method of recommending my own practice to the adoption of young practitioners, if I had summarily stated that the practice of all those gentlemen is, in my opinion, both erroneous and dangerous.

These quotations may appear prolix to the reviewer, as he may be familiar with the works in question, but young practitioners, and especially those who do not reside in large cities, cannot be expected to have access to those works.

Besides the twenty-seven lines of directions, I have added four or five pages of illustration, which it is probable the reviewer never read.

So far from my directions being confined to supporting the perinæum with the naked hand,

and smearing the parts with soft lard, my very first direction is one which I suspect is seldom steadily pursued. It is in the following words : —“ From the time that the head of the infant clears the os uteri, the practitioner is to remain by the patient.”

Of the importance of this precept, no better illustration can be given than the records of the cases in the Dublin Lying-in Hospital, inserted in Dr Collins's valuable publication. Will the reviewer venture to allege, that if that rule had been adopted the following case (which I have selected out of several similar ones, chiefly on account of its brevity), could have happened?

“ No. 526 (Collins, p. 470) was reported to have been twenty-four hours in labour before admission. About twelve hours after she came in, it was discovered that the face was turned towards the pubes, and pressing so strongly on the urethra, the catheter could with difficulty be passed. The pains continued strong for

fifteen hours from this time, yet the head did not advance. It was found advisable to lessen it." Now, I ask the reviewer if any practitioner of common humanity could have sat witnessing for twenty-seven hours the unavailing sufferings and tortures of a poor woman? The narrative clearly proves that she had only been visited occasionally.

The next direction is very summarily stated, occupying nine lines, which is probably the reason that its value has been overlooked. It is to make counter pressure on the perinæum, without the intervention of a soft cloth, and the reason urged for this deviation is, that it is obviously impossible to afford the proper support, if a cloth be interposed, a circumstance which had been explained in my remarks upon the practice of Dr Denman and others. In point of fact, every man who has attended to the subject must know, that in order to protect the perinæum, the counter-pressure must be varied, not only in different women, but in the same woman in different labours.

Certainly the reviewer is correct that I advise the use of soft lard for the purpose of promoting the dilatation of the passages—of alleviating the sufferings of the patient—and of preventing subsequent inflammation and swelling of the parts, and I ask him if these are not most important considerations.

But I have inculcated a fourth practice, to which the reviewer has not alluded, and that is, forcing forwards the perinæum towards the pubes during every pain after the head begins to protrude. This practice I have long recommended, from my conviction of its utility, but I am aware that my friend, Professor Burns, entertains a different opinion on this subject.

The truth is, that I was led to give directions for the management of the second stage of natural labour, as I have incidentally stated in my Second Part (page 101), in consequence of the numerous cases on which I have been consulted, both from England and the Continent, where prolapsus uteri had been occasioned by

laceration of the perinæum during labour. I was perfectly aware that selfish or prejudiced practitioners might be unwilling or unable to adopt the means which I recommend for the protection of the perinæum, and that persons anxious to decry the practice of midwifery by medical men, might hold out such directions as indelicate. But I certainly could not have supposed that those directions could have been so misrepresented in a journal edited by a respectable physician.

VI.—The comments which I have hitherto felt it incumbent upon me to offer, have related to important practical points. It remains for me only to advert to the observations of the reviewer in a note, page 50, which I do rather in order to vindicate the truth and accuracy of my own statements, than to shew the *mala fides* of the reviewer, which is so very obvious that it is most wonderful he should have displayed it with so little reserve :—The words to which I allude are these :—“ Dr H. says, that in his long ex-

perience he has in no instance found the pulse of the child, before it begins to breathe, to exceed sixty beats in a minute. Of the truth of this he has repeatedly convinced himself by counting the pulsations of the cord, not only while the child was still in utero, but also after its expulsion before respiration commenced. The experience, however, of his friend and coadjutor, Dr Moir, and we need scarcely add, of all other accoucheurs, is directly opposed to the Professor's statements."

Now, I have to state, in the *first* place, that the reviewer has totally forgotten that the professed object of my work is to record my own opinions and discoveries, and therefore, although he and all the rest of the profession were to declare their disbelief of the fact of the slow action of the foetal heart previous to breathing, I should certainly prefer trusting to the testimony of my own senses, to any appeal to the opinions of others.

Many years have elapsed since I mentioned,

in lecturing, the discrepancy between the action of the foetal heart previous to breathing, and the action of the heart of the parent, as a curious fact, of which physiologists had not been aware, since it furnishes one of the strongest evidences that there can be no direct vascular communication between the mother and the infant. To my surprise, this fact was, about from ten to fifteen years ago, alluded to in one of the articles in the *Dictionnaire des Sciences Medicales*, which I have no leisure at present to investigate, as the fact is the only matter which your reviewer has been pleased to question.

Secondly, The assertion of the reviewer, that the experience of Dr John Moir is directly opposed to the Professor's statements, is a perversion of a recorded fact, which I shall not stoop to characterize. Dr Moir, in reporting his experiments with the stethoscope, as printed in my Appendix, has thus expressed himself, page 312 :—" This effect of the contractions of the uterus acting indirectly on the

circulation of the foetus through the medium of the brain, may account *for the fact, that the pulsations of the heart are only about 60 in infants who do not breathe on birth, but in whom the circulation still goes on through the chord.*”

By this quotation, it is evident that Dr Moir bears testimony to the truth of my account of the action of the foetal heart before breathing ; while your reviewer avers that the experience of Dr Moir is directly *opposed* to the Professor's statements.

Thirdly, After reading Dr Evory Kennedy's Observations on Obstetric Auscultation, I requested Dr Sidey and Dr Moir, both of whom are engaged in very extensive practice here, to attend particularly to this subject, and to endeavour to ascertain the number of pulsations in the umbilical arteries of the foetus, in cases where they could reach the cord before the birth of the infant, or where the infant did not breathe when born, although the action of the heart continued. On reading your review on

the 26th October, I wrote to those gentlemen, to request them to communicate to me the result of their observations on this subject, and the following are the letters which I received:—

2, HERIOT ROW, EDINBURGH,
27th October 1836.

DEAR SIR,—In answer to your note of last night, in regard to the action of the foetal heart before breathing is established, I beg to state, that since you directed my attention particularly to the subject, I have had an opportunity of ascertaining, in eight cases, that the action of the heart is only sixty or under it in a minute, before the act of respiration is established.

In four of the eight cases, I had to deliver the patient by the operation of turning. During the absence of the labour pains, I had an opportunity of deliberately counting the pulsations of the cord, and of contrasting them with those of the iliac arteries of the mother, which I felt distinctly through the parietes of the uterus. I am, Yours very truly,

(Signed) CHARLES SIDEY.

5, GEORGE STREET,
26th October 1836.

MY DEAR SIR,—In reply to your letter of this date, I beg leave to say, that till the publication of Dr Evory Kennedy's book on obstetrical auscultation, I considered that the slow action of the foetal heart in those cases where the infant does not breathe upon birth, though the circulation continues, had been a fact universally acknowledged by the practical part of the profession.

On reading Dr Kennedy's publication, my attention was very particularly directed to this subject, and I can, with great truth, assure you, that my observations since that time have invariably confirmed the fact, that the pulsations of the heart are only about 60 in those cases, as mentioned in my former communication to you, and recorded in the First Part of your work, page 312. I am, dear Sir, yours very truly,

(Signed)

JOHN MOIR.

2, HERIOT ROW, EDINBURGH,

October 29, 1836.

MY DEAR SIR,—Since I wrote to you on the 27th, I have had two opportunities of ascertaining the action of the foetal heart previous to the commencement of breathing. The one occurred last night, and the other this morning.

In the former case, I felt the cord of the infant round its neck whenever the head filled the pelvis, and pressing my finger upon it, I repeatedly counted the pulsations, and I found them to be rather under sixty in the minute.

The infant whom I assisted into the world this morning, did not breathe for some little time, but the arteries of the navel string beat distinctly. The number of pulsations in the minute varied from fifty-six to sixty. I ever am, my dear Sir, yours truly,

(Signed) CHARLES SIDEX.

These observations, will, I trust, satisfactorily expose the misrepresentations of which your reviewer has been guilty, and will enable your readers to determine what degree of credit is due to the animadversions of such a person.

I am,

SIR,

Your obedient Servant,

JAS. HAMILTON.

EDINBURGH,
8th November 1836.

the first of these is the fact that the
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